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THE HUNTERIAN PREPARATIONS IN THE MUSEUM OF THE ROYAL COLLEGE OF SURGEONS, LONDON.

[SIR CHARLES BELL has recently delivered a course of lectures at the London College on the Hunters and the Hunterian preparations. We observe that, in his first lecture, Sir Charles has followed up some of those new views in physiology which he developed so admirably in his treatise on the hand. The following extract from this lecture, which we obtain from the report in the Lancet, will be read with interest, both by those who have perused this treatise, and by those who still have that pleasure in store.]

"Now, gentlemen, to enter upon the subject of structure. The first thing we come to is bone. Last year I dwelt long, perhaps tediously, upon this fact—that man and all animals bear a peculiar relation to the globe on which we stand; and, if you recollect, I illustrated this position by reference to a piece of machinery. I stated to you that a machine of the simplest construction for raising a weight, is said to multiply the powers of the hand, and I represented to you—as saying that we did not comprehend how the powers of the hand could be multiplied. If you go into this inquiry, you will find that the power of the hand is not multiplied, and that the machinery for moving heavy weights has no power at all, unless it be screwed to a root which is fixed to the ground; for it is from the resistance which is afforded at this point of fixation, that an addition is made to the power of the hand, and that we are enabled to raise a great weight. If you take a simple lever (for all the wheels and complex parts of a machine are but various forms of lever), you will find that it has no power unless there be a fulcrum or point on which it can be rested. Now the point of resistance gives it a power apportioned to the strength of that resistance. Now every limb in the human body is a machine. It is not enough that the limbs have got bones, muscles, and nerves to move them. How is this human machine fixed? for unless it be fixed, it can be no machine. But it *is* fixed, and it is fixed by the weight of the body, and if you were to pursue this subject, you would find that the body bears the same relation to the globe

on which we stand, that the lever of a machine does to its fulcrum or resting point. At first sight this may seem extravagant; the fact is, however, that the weight of the body corresponds with the attractive power of the earth upon that body, and if you fix this fact in your mind, (and it is an undoubted fact), you will not be surprised at the statement that the animal body bears a relation to the whole globe.

This brings us to the subject with which we started. The bones of animals must have a point of resistance—they must resist external impulse. The functions of assimilation, circulation, absorption, and respiration, are performed by organs which must be united together, and yet possess a strong means of resistance against each other. To be sure in the medusa, and such animals as are floating in the water, you do not need all this texture of bone; common cellular texture is sufficient in these instances, because the animal is buoyant: but if it were to remain out of water, and rest upon particular points, or upon an extremity, that extremity must have a power of resistance proportioned to the animal or muscular power contained within: and, in fact, you will find that throughout the whole of nature there is such a provision as this for each animal.

I ought to mention that this is an idea of Mr. Hunter's. In the specimen which I now show you, there is no interior bone, but there is a provision for the protection of the animal. In the specimens which are now before you, you have two conditions of this earthy matter, one internal, one like a shell to protect the other, as in common coral. There is, in the skeleton, a column, and that column is requisite to ensure the existence of the exterior delicate animal texture. Now such you will find to be the case in all creatures. Take the larva of an insect—one of these animals. What is there here? observe that this is the language of Mr. Hunter. It is what he tells you. Each of these bottles presents to our notice a fact. The arrangement of these bottles therefore becomes most important, and it is therefore most important that you should retain a knowledge of the principles and arrangement of Mr. Hunter, because they constitute a language. They are Hunter's thoughts, and the arrangement of them forms a universal language. It is clear that he meant to say, that even in the lower animals, there must be some crust, some protection for the animal, some outward deposit which should give them the power of that resistance. You find this fact to be universal. Why? The animals could not move without it. Hence those animals which have the hardening material in a series of rings, have attached to these rings a succession of muscle by means of which they use them. I go with this view then that I present to you the larva of the fly which fixes on the willow. Now, gentlemen, you have read, I doubt not, with great admiration, the large and beautiful volume of Albinus, and the fine quarto dissertation that accompanies it; but as fine a work, as elegant a work, in all its parts, has been published on the muscles of this animal, the willow caterpillar. You will find that these rings give the animal the power of resistance. They consist of depositions of hard materials. They are one upon another, and admit of motion. Take off these rings, and you will find muscles running along the whole extent of the animals. I am anxious to show what Mr. Hunter had in his mind. Here is a pre-

paration exhibiting that which others have dwelt upon with so much interest. Take off this general muscle, and you find that other layers are beneath it. Then take off those layers, and you find others still beneath them. You are not entitled to suppose that these are accidental arrangements. Do not presume, that because they are minute, they are accidental. There are 500 muscles attached to this hard ring, which passes round the animal, each muscle having its nerve, which I notice just to bring to your recollection that each nerve has its source of energy. Now, if I take this opportunity of making a comparison, let me ask whether there be any part of man which presents a complication equal to this. There are the powers of the hand and the action of the muscles, but the source is from the brain, the circulation of the heart, the organization of the lungs. Is that member simpler or more complex than is the structure of this animal? Here is a breathing tube along the whole of this animal. Take each portion of it. There is a distinct trachea with each portion, a distinct heart with each portion, new, digestive, and respiratory organs with each portion. Now looking at these inferior animals, are we entitled to consider them as deficient in organization—as early attempts of the Creator?

But if you take the larva of an insect, and observe its degree of perfection, you will still find the same matters or circumstances which I have mentioned, though with a beautiful variety of structure. It is for this purpose that I show you this sketch of the leg of a beetle. This subject is connected with the bone of the animal now under my hand. When you have passed many years in the study of osteology, you acquire a notion that bones are the perfection of animal structure. But this is by no means the fact. If it were to constitute the internal skeleton for the external skeleton, the articulation of the internal skeleton must be deficient upon the principle of mechanics. It is a familiar fact, that when the matter of bone is removed, the transverse fracture is morbific. You do not need that I should now make that demonstration to you; but if mechanical strength be increased in proportion as the matter of bone is distant from the centre of the whole animal, the perfection of strength is accomplished in the insect, for the bone is external.

Then, again, bone must be part of the living frame; for you well know that it must be absorbed, that it must be deposited, that it must undergo all the changes which belong to a living part. Therefore it must have vessels, it must be porous. But in external shell, in the external bone which protects an insect, the substance may be very dense, because there is no condition implying the necessity of change by means of the operation of arteries and veins, so that compared with bone it is firmer in mechanical structure, and better, as it were, in its intimate substance. We go beyond this, gentlemen. Be assured, that there is always a correspondence between the animal, the muscular power, and the resistance. You may observe it in the bones of man, and in the bones of race-horses as compared with draught-horses; or you may observe in insects that the texture, the firmness of the scales, acting as bone, is indicative of muscular strength. Indeed the muscular strength of insects has been notorious since the time that certain wags cast ridicule upon Socrates for watching the strength of an insect. That philosopher knew, and every

one else knows, that the muscles of these animals possess an extraordinary degree of strength. No wonder that it should be so when you observe their apparent complication, for it is no more than *apparent* that there is no complication. Thus we find that in the covering of insects in the articulati, if you notice, they are finely articulated; the processes in the thigh-bone of man are not more regular, nor more curiously exact, nor so much so, as are these inwardly-projecting processes. Here is the external bone, the processes passing within may be considered as the thigh-bone, and the shell projecting outwards may be considered as the tibia and fibula—in that covering, as I said, there is a great mechanical advantage from the attachment of the minute muscles.

Now I state these things, gentlemen, to show you to what an extent the consideration of the bones of animals may be carried if you contemplate them in all their bearings. If I have formed a correct opinion on this subject, it is, that it will be more advantageous to endeavor to complete one subject than to seek your approbation by passing over many subjects. I have observed, that men of thought and ingenuity are more fatigued—an effect which should be avoided—by contemplating a variety of subjects, than by following up one only with great minuteness.

Let me then to-day conclude by observing, that studying these things tends to prepare your minds for other and better avocations. I say again, that natural history or comparative anatomy is but the means of preparing you for other branches of the profession. Now it is acknowledged that on many subjects, mathematics is an excellent study for strengthening the mind and acquiring the power of concentrating the mental faculties on any subject; but that will not serve our purpose. Your business is to observe the phenomena of life, and there can be no better way of preparing your mind to observe facts, and to follow them up by exact reasonings, than by taking the method which our great authority Hunter has done before us. It is by following up comparative anatomy and natural history that we discover the importance of the human structure, and thus impart a dignity to our engagements, by making every effort tend to the alleviation of human suffering."

DR. LATHAM'S CLINICAL REMARKS ON THE DURATION OF FEVERS.

All the good old notions in medicine are fast disappearing before the modern rage for precise induction and morbid anatomy. The doctrine of critical days in fevers has been for many years consigned to the tomb. But the belief in specific periods of duration has survived, and old practitioners of the present time yet talk of fevers of twenty-one and of fourteen days, as if their existence were beyond possibility of doubt. The able and the intelligent smile at the fancy, and scarcely consider it necessary to deny, what they do not feel seriously called on to believe. Some tables, constructed by the active observation of Dr. Latham, will serve to show how futile is the popular creed in this particular instance.

It has been remarked by a keen observer of human nature, Gibbon, that a dogma, intended to exercise a wide and a permanent influence on mankind, must not appeal too clearly to the understanding. What all

can comprehend and test by their common experience, implies no peculiar faith nor extraordinary merit in the believer. We suppose that this principle will sufficiently account for the hold that such doctrines as that of critical days, have retained on the minds of physicians, from the time of Hippocrates to this. The few sober statements of Dr. Latham will complete the discomfiture of this ridiculous assumption.

Dr. Latham justly remarks that it is not always easy to say when a fever ends, or on what precise day it begins. The period of accession is, however, more commonly distinct than that of decline. A decisive phenomenon, or train of phenomena, as rigor, or sickness, or headache, may give precision to the former, whilst the latter has no such distinctive mark, and illness gradually merges in convalescence. This objection will appear so obvious and so striking to those of any experience, that we need not dwell upon it further. We must be content, in the generality of cases, to note that about such a time the patient began to get well, and it is on such data for calculating the termination of the malady, that the table we shall now introduce is founded.

Of 309 cases of fever, in Bartholomew's Hospital, there were twelve of which the duration could not be satisfactorily determined. The remaining 297 terminated on the days expressed in the table.

Days of the Fever.	Number of Cases ending on each day.	Days of the Fever.	Number of Cases ending on each day.	Days of the Fever.	Number of Cases ending on each day.	Days of the Fever.	Number of Cases ending on each day.
5	2	20	9	34	6	48	1
6	3	21	8	35	5	49	8
8	3	22	7	36	1	50	1
9	6	23	9	38	3	51	1
10	3	24	10	39	5	53	2
11	12	25	6	40	8	55	2
12	13	26	4	41	3	56	1
13	12	27	7	42	4	57	2
14	8	28	8	43	2	59	1
15	9	29	4	44	3	60	1
16	14	30	11	45	1	62	1
17	12	31	11	46	4	65	1
18	16	32	6	47	1		
19	8	33	8				

Dr. Latham makes the following observations on the foregoing list.

"Taking, then, the numbers exactly as they are represented in the table, it would appear that fewer cases terminated on the 14th and 21st day than on several days both prior and posterior to each. Out of two hundred and ninety-seven cases, eight only terminated on the 14th, and eight on the 21st.

For a fever to end before the 10th, or to be protracted beyond the 31st day, is unexpected and unusual. Thus a range of twenty-two days embraces the period within which the majority of cases come to their close; and, within this range, the table shows that there are twelve days (above half) more favorable to the termination of fever than either the 14th or 21st. Two equally favorable, and six only less so.

But let us allow a still larger latitude in seeking to know whether fevers are apt to come to their close *some time about* the 14th or 21st day. Let us take a range of three days, by joining each of these with the day im-

mediately before and the day immediately after it; and then, adding together the number of cases which ended on the 13th, 14th, and 15th, and on the 20th, 21st, and 22d, let us see what proportion the sum bears to the number ending on any other three days taken consecutively.

Thus on the 13th, 14th, and 15th days, taken together, twenty-nine cases terminated; and on the 20th, 21st, and 22d, taken together, twenty-four cases terminated. These are our standards of comparison. But on the three days preceding our first standard of comparison, viz. the 10th, 11th, and 12th, twenty-eight cases terminated; and on the three days succeeding it, viz. the 16th, 17th, and 18th, forty-two cases. Again, on the three days preceding our second standard of comparison, viz. the 17th, 18th, and 19th, thirty-six cases terminated; and on the three days succeeding it, the 23d, 24th and 25th, twenty-five cases.

Finally, then, from the event of these two hundred and ninety-seven cases, no proof can be derived that there is any law of fevers inclining them to terminate upon one particular day more than another, or even *some time about* a particular day, whether it be the 14th or 21st, or any other day."

From all this it appears that the duration of fevers is indefinite. Dr. Latham has the merit of pointing out, in a methodic manner, what most men of sense and experience were sufficiently aware of.

Facts are so much superior to reasons, that any further consideration of the subject would be profitless and tiresome. But we cannot forbear from urging one point which would almost be sufficient of itself, to prove that fever can evince no determinate duration. At the time when the subtle Greeks gave laws to the progress and continuance of this affection, they were not aware of the organic lesions that attend, if they do not produce it. But the examination of the bodies of the dead, has shown that ulcerations of the mucous membrane of the bowels, and organic alterations in the chest and in the head, are observed in a greater or less degree, in the majority of cases. Our knowledge of such alterations, precludes our belief in the possibility of their displaying particular days of decline and cure. An extensive ulcer of the aggregated glands of the ileum must cicatrize as other ulcers do, and the period of its cicatrization must be greatly modified by the nature of the treatment, the constitutional powers of the patient, and by other influences, frequent in occurrence, and powerful in operation.—*Medico-Chirurg. Rev.*

FUNGUS CEREBRI—WITH A CASE.

[Communicated for the Boston Medical and Surgical Journal.]

In some instances, when an injury has been done to the brain, producing a lesion of its substance; the case having, perhaps, proceeded favorably for a few days, or even weeks, the surgeon, then, upon exposing the wound for the purpose of dressing, will be surprised to see a substance somewhat resembling the brain in appearance, and pulsating like that organ, protruding from the perforation of the cranium. This peculiar affection has received the appellation of Fungus, or Hernia Cerebri.

This tumor shoots up rapidly in most cases; so that in a few days

from its first appearance, it will have arisen one or two inches above the external surface of the skull, and projected proportionably on all sides. From the surface of this fungous mass, a fœtid sanious fluid generally exudes; and when the part is handled, appears readily inclined to bleed. Most medical writers agree in the opinion that this disease is one of extreme danger; but as to its nature and treatment, a very great diversity of opinion exists. Mr. Abernethy has attributed the complaint to coagulated blood, and Dr. Dorsey to an abscess in the brain; a portion of which organ, by either cause, is thought to be pushed up by the accumulating matter below, and thus to occasion the tumor. Where fatal cases of this kind are examined, post-mortem, either collections of blood or purulent matter, generally, are found. Here appears to be one cause of the diversity of views on this subject; and the surgeon who makes the examination is led to believe that one or the other of these causes produces the disease, just as he may chance to find the parts on dissection. But are not one or the other of these appearances found in almost all fatal cases of cerebral injury, even where no fungus had existed, and consequently furnishing, where it did, but uncertain evidence whether either of them was the cause of its production.

The views of Dr. Charles Bell, as to the *nature* of fungus cerebri, exclusive of what he has admitted from mere courtesy (to his friend Mr. Abernethy), go to prove most conclusively that these tumors are organized bodies. The reasons which he exhibits to support his opinion are, 1st. "That the surface of the tumor bleeds when torn or cut. Not only it bleeds if it be torn off, but the abraded surface bleeds. This is not like a coagulum. 2d. It shrinks and collapses upon death; which is certainly a mark of a fluid having circulated within it. 3d. I have," says he, "a preparation of this disease, where an ulcer passes from its base into the lateral ventricle, and where the ulcer communicated outwardly, and yet no drop of blood or coagulum was seen upon the surface of the brain, or in the cavities. 4th. It is not formed of concentric laminæ, as the coagulum of an aneurism is. The blood never bursts from its surface, as it would do even from a venous tumor, which had power in the first place to burst the membranes of the brain. It is affected like spongy granulation by caustic. A degree of compression equal to the compression of a considerable artery will not subdue it, when its growth has got a-head. 5th. It has a fibrous structure; and when it is dissolved in death, it hangs in shreds, not like a coagulum." In addition to these, there is one other reason, which was exhibited in a case of this kind which lately came under my observation, viz. an ulceration and sloughing of a portion of the tumor, leaving a healthy granulating surface; a fact which could not exist in an unorganized substance.

It becomes a matter of great importance to ascertain the true nature of this disease, in order to arrive at a correct method of treatment. The brain is an organ of peculiar structure. Although extremely delicate in its texture, it is supplied largely with blood, moving with constant pulsation; and when the restraint of the cranium and membranes is removed, and the organ excited by inflammation consequent to lesion of its substance, we may suppose there exists a natural tendency to throw up loose and imperfectly organized matter from its abraded surface.

The inflammation which ordinarily accompanies fungus cerebri, is probably a frequent cause of death, independent of this disease. But when the growth of this tumor is unrestrained, or goes on increasing in size, its final termination is found to be fatal. Its extensive growth appears to exhaust the energies of the brain. To restrain its growth, then, is the first indication of cure. This can only be effected by using an extreme antiphlogistic course of general treatment, and by the application to the tumor of some hard substance, with a sufficient degree of pressure to overcome its tendency to enlargement.

The second indication is to obtain the removal of the tumor. This has been attempted with caustic; but I believe always with fatal effects. Dr. Charles Bell recommends that the tumor should be cut freely off, and then light and equable pressure applied to the surface. I am not able to state whether this method has ever been successfully tried. It is believed that the best means of obtaining its removal is by the effect of pressure applied steadily to its surface; for as soon as its tendency to increase is overcome, it seems to lose its power to exist, and soon falls off.

Should symptoms of cerebral compression supervene to the use of pressure on the tumor, the force should be lessened for a time; and if relief is not obtained, a lancet should be pushed just within the edge of the cranium at the base of the tumor, as directed by Mr. Hill, to permit the escape of any matter which may chance to be there confined.

In illustration of the views herein expressed, the following case is respectfully presented.

On the 19th of August 1833 I was called to visit G. M., a boy four years of age, who had received a kick from a colt, upon the os frontis, about one inch above the right superciliary ridge. The skull was broken, and the fragments were driven through the membranes into the brain, portions of which were discharging from the wound. Two pieces of bone, each about one inch in length and half an inch in breadth, were drawn out from the brain; all the smaller portions carefully removed, and the wound lightly dressed with lint and bandage. Both the delirium and bleeding, which had been considerable during the operation, soon subsided. The patient was kept at rest, and restricted to a light use of gruel, a drink of cream of tart., and an occasional cathartic of sal. epsom. For several days the case proceeded very favorably. On removing the dressing from the wound on the sixth day from the injury, I observed a fungous tumor about half the size of a hen's egg, protruding from the fissure, and extending to some distance on the outside beyond the opening in the cranium. It had some resemblance to the substance of the brain, though a little darker in color, and pulsating like that organ. The surface of this fungous tumor was moistened with a fœtid serum, and bleeding was produced by the lightest handling. The day following I found the tumor doubled in size, and presenting a most formidable appearance. No delirium existed, but the pulse was more frequent, and the general symptoms of the patient were less favorable. I prepared from the bowl of a pewter spoon, which was at hand, a plate sufficient to cover the whole tumor. This was attached to a bandage, and its concave side applied to the tumor, having previously laid on a thin dressing of lint and serate. Care was taken in applying this compress not to

draw the bandage so tight as to give much uneasiness. The tumor did not increase much after this application was made. In four or five days, ulceration and sloughing had removed the major part of the tumor. The surface that remained was extremely sensitive, tolerably healthy in appearance, but still more elevated than the cranium. The plate was then made less concave, and reapplied for four or five weeks; the wound was contracted with adhesive plaster, and in the mean time continued to heal. The plate was then entirely discontinued, and the integuments, which had formed some troublesome adhesions, during the protrusion of the brain, slowly closed over the wound, and I had the pleasure of witnessing the recovery of my little patient; his intellectual faculties affording the very satisfactory evidence of having received no permanent injury.

Goshen, Conn. January 22, 1834. SAMUEL W. GOLD.

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SUCCESSION OF THE ERUPTION OF THE TEETH.

SINGULAR as it may appear, there exists a diversity of opinion among medical writers respecting a point so easily determined as the order in which the infant teeth emerge from the gums. Any one would suppose that a mere matter of fact, and one of daily observation, were the last thing in the world to divide the opinions of medical authors. Yet even on this simple fact there are contending views, and of late there has arisen in England, a public discussion. Now let us all observe for ourselves, and, for ourselves at least, let us settle the question. It is too often the case that matters of moment are regarded exclusively worthy the study and observation of the student and practitioner. Diseases of magnitude and severity—points in physiology or pathology bearing an important relation to life, are investigated with due care, and receive from the practitioner the share of reflection and effort that they evidently require. Not so with subjects of minor importance. Although they form an integral part of a thorough medical education, they are too often overlooked, or else they are regarded as too insignificant to occupy the attention of a liberal professor. Nothing can be more true than that small diseases, like small sins, affect the comfort and real happiness of individuals as much as greater ones; and it is as much the duty of the physician to make himself acquainted with the less important details of human physiology, and the less imposing of the ills that are our common inheritance, as with the philosophy of circulation and digestion, and the theory and treatment of fever, dropsy and the cholera. We are acquainted with a gentleman of rare genius and profound professional learning, whose services were sought with avidity and appreciated most highly, but who soon lost an extensive and lucrative practice by a uniform neglect of small matters in the way of his profession: lofty in his notions and conscious of his ability, he would never condescend to notice a whitlow or prescribe for a chilblain. The families in which he practised soon found that in a large majority of the cases in which they called in their physician and looked to him for aid and advice, he really knew little of the case, and

affected to know less. Notwithstanding, therefore, his acknowledged ability and skill in the most rare and important cases, they were compelled by an ordinary regard to their own comfort and that of their children, to seek the services of a less gifted but less aspiring and more useful physician. This indeed was an extreme case, and one we have occasion well to remember, for the individual was a personal friend; but in a greater or less measure we all err, and we all lose practice, and we all abridge our usefulness, in the same manner. The little articles of food that are so grateful to a patient during the early days of convalescence from severe sickness, every practitioner should know how to prepare, and neglect not to direct; and there is no *disease*, though it be but a corn, a wart, or a pimple, that should not be made a subject of study, and an object of serious attention wherever it occurs.—Among small matters on which medical men are frequently questioned, is that which we have placed as a title to these remarks, and it is to the kind of neglect alluded to, we attribute the fact that many among us have no answer at hand; and of those who have, some will give a different reply from others.

Most writers have agreed on a certain succession in the eruption of the teeth, but this agreement has resulted probably from the fact, that each has been content to rely on the authority of his predecessor in a matter of comparatively so little moment. That this is the case, is rendered probable by the observations of the late Sir Richard Croft and some later writers, who have stated an entirely different succession as that which usually prevails. Our intention is to offer a tabular representation of both views of the subject, in order that each reader may observe for himself, and thus lead the way to a correct statement of the facts.

By this first table it would appear that the teeth in the lower jaw precede those in the upper.

Molars.		Canine.	Incisors.				Canine.	Molars.		
10	6	8	4	2	2	4	8	6	10	Upper Jaw.
9	5	7	3	1	1	3	7	5	9	Under Jaw.

The more recent opinion is, that after the appearance of the central incisors, those of the *upper* jaw appear first, as in the following table.

Molars.		Canine.	Incisors.				Canine.	Molars.		
9	5	7	3	2	2	3	7	5	9	Upper Jaw.
10	6	8	4	1	1	4	8	6	10	Under Jaw.

ARROW ROOT.

The necessity of possessing means for distinguishing adulterated from genuine articles, both in food and medicine, is fortunately much less with us than in England or France. We are not very likely to have our bread whitened with alum, emulsion of almonds substituted for milk, or plaster of Paris sold us for cream of tartar, because those whose business it is to prepare and furnish these articles are not compelled, by hard necessity, to adopt these dishonest means of procuring themselves a subsistence. But as every rule admits of exception, and as those who vend to consumers are not always good judges of the purity of the article, it is well that the means of obtaining their test, in regard to those of frequent use, should if possible be made known. It is a fact understood both by purchasers and by dealers, that a large proportion of what is commonly sold under the name of arrow root is not in reality obtained from the maranta,

but consists of the fecula or starch from the potatoe, and on being prepared for use, exhibits not a little of the peculiar taste of this vegetable. The resemblance, however, between the real and genuine arrow root, is such as may easily deceive the inexperienced; and although a decidedly different sensation is communicated on tasting of the two, it is a difference which hardly admits of precise description. We have lately met with a more particular account of the physical qualities of the true roots as distinguished from its counterfeits than has, we believe, before been published, and which we think cannot fail to prove interesting to our readers. It appeared at first in Raspail's Nouveau Systeme Organique, and is translated in the first number of a London quarterly Journal of Medicine, few copies of which we imagine have reached this country.

"Arrow root," says Berzelius, "being thought strengthening by some physicians, is sold very dear, for which reason it has been attempted to distinguish it with certainty from other kinds of starch. According to Guibourt it can be recognized under the microscope, by the grains of arrow root not being transparent, and smaller than those of potatoe starch, though their shape and size are variable also." Though I cannot help congratulating Berzelius on his newly-born tolerance of microscopic observations, still I must lament the complaisance which induces him to register, in catalogues made with the authority of his name, such superficial observations as those which he takes from Guibourt. According to the character attributed by this writer to the fecula of arrow root, there are perhaps a hundred vegetables in France, whose fecula might be confounded with this Brazilian substance. What fecula is not transparent; and what fecula is more transparent than that of the solanum? Moreover, what fecula, with the exception of the fecula of chara seeds, has not smaller grains than the fecula of potatoes, and of a size quite as variable? As to shapes, how many are there whose shapes are infinitely varied? But by an unlucky chance it happens that so far from being transparent, the grains of arrow root are more shadowed than any which we have observed, and present characters which we have not met with in any other. These marks of distinction are as follows. The fecula of arrow root, when examined in large quantities, has a crystalline yet faint lustre; it is rougher to the touch than that of potatoes, and almost as much so as that of wheat starch; it contains small clots which resist pressure, and crackle under the fingers. When examined in water and by the microscope, it presents groups of five or six, and even of ten or twelve grains, which the most rapid movement and the most prolonged shaking do not succeed in discovering, but which continue to float over the liquid in company.

But the most distinguished of all the physical characters of this fecula, is that each grain is the half, or quarter, or third, &c. of a solid sphere; that others are small cylinders with one extremity rounded en calotte, and the other flattened; that others exactly resemble a painter's mallet. So that each of these grains has one or more angular surfaces, whose refraction produces those strong varied shadows which we observe on the centre of the microscopic image. One might sometimes suppose one's self to be looking at crystals. Their structure is such that it may be better known from a written description, than from the most exact drawing. Moreover, one often sees, through their transparent side, black lines crowning one another, sometimes like a T, and sometimes like a star, just as in the fecula of rye; and if we make the grains turn round by moving the water, we can assure ourselves that these lines are by no means su-

perficial, but on the contrary exist in the very heart of the grain, indicating the existence of cells, like those I have observed in the lentil; the largest grains do not exceed one twenty-fifth of a millimetre in diameter. The adhesiveness of a great number of these grains to one another, and the angular surface which they have contracted by their agglutination, (always preserving, however, one of their curved surfaces), would lead one to suppose that this fecula, which is composed of round and softish grains, has been treated immediately after its extraction by a violent stove heat. What confirms me in this supposition is, that the long boiling, which is sufficient to spread out the integuments of the potatoe fecula, so as to make them acquire twenty or thirty times their original diameter, barely quadruples the volume of the grains of arrow root. This explains how it is that Pfaff found that ten grains of arrow root boiled in an ounce of water, merely produce a mucilaginous liquid, while the same quantity of common fecula in the same quantity of water, forms a gelatinous, or real starch.

DR. O'BEIRNE'S NEW VIEWS ON DEFECATION.

Although Dr. O'Beirne's *views* were advanced some months since, and in the rapid succession of novel theories, can hardly be regarded as new, yet as the interest which they excited seems not to have altogether ceased, we think it due to our readers to make them the subject of a short article.

Dr. O'Beirne, then, differs from other physiologists in the function which he assigns to the different portions of the larger intestines in producing the act of evacuation. It has been generally believed that the feces gradually passing through the cœcum, and colon, and becoming more solid in its progress, was finally lodged in the cavity of the rectum, where accumulating by degrees, and acquiring still greater solidity, it at length produced irritation and the desire of expelling it; and that this was accomplished in part by the muscular action of the rectum itself, and in part by the abdominal muscles, the contraction of which tended to lessen the cavity of the abdomen generally, by forcing out its contents. In this view, then, the rectum is considered as always containing some portion of fecal matter, and as constantly receiving it from the sigmoid flexure of the colon, thus acting as the reservoir or place of deposit to the rest of the canal. This view of the subject has been thought to be amply confirmed by an examination of the structure of the parts, the rectum being found to be the largest in diameter, and well calculated by its direction along the sacrum, to act conveniently in freeing the system from the accumulated feces.

To this view of the subject, however, Dr. O'B. does not accede. He conceives the rectum to consist of two parts, the pouch, or that nearest the vent, the natural state of which is open and dilated; and the higher part, which is always contracted, so as to bring its parietes into contact, except at the moment when the evacuation occurs. At this time he conceives the feces to be partly contained in the sigmoid flexure, and partly in the cœcum; that during the process of evacuation an action takes place, by which the sigmoid flexure ascends from the cavity of the pelvis into the left iliac fossa; and in proportion to the rapidity and degree of its distension, begins to turn upon the rectum as upon a fixed point; until at length it directs its greater arch forward and upward, and its lower backward and downward; by which movement the contents are brought

perpendicular to, and so as to bear directly upon, the upper extremity of the contracted rectum; that their weight being insufficient to force a passage, they remain stationary till the increasing accumulation produces uneasiness and contraction of the abdominal muscles and diaphragm; that then the contents of the distended flexure are forced against the upper extremity of the rectum, open its cavity, are urged into it, and descending into the pouch, occasion an ardent desire to go to stool, and a nisus, by which the sphincters are forced open, and the final expulsion effected.

Dr. O'B. farther maintains, 1. That the cœcum is considerably distended before it is unloaded. 2. That the whole mass by which it is distended, and no more, is transferred at each time that it is unloaded. 3. That, at the moment of going to stool, there is generally one mass of fecal matter in the cœcum, and another in the sigmoid flexure, and, consequently, that these may be considered as the measure of the quantity discharged when the bowels are said to be freed. 4. That as two distinct acts of expulsion are always required before a healthy person finds his bowels sufficiently freed, the capacity of the cœcum may be received as the measure of that of the rectum. Lastly, that the *sphincter ani* muscles are merely subsidiary agents in retaining the feces.

In support of these doctrines, Dr. O'Beirne adduces the following facts and arguments;—that the accumulation of excrement in the rectum would derange the ordinary functions of the bladder, and, by irritating the *sphincter ani*, prevent man from retaining his feces;—that "the circumstance of nature forming one of her chief depots of excremental matter in a part of the intestinal canal so close to, and continuous with, the rectum, as the sigmoid flexure is, appears altogether inconsistent with the idea of a free passage between these portions of the canal;"—that there is considerable opposition to the ascent of an enema even under the most favorable circumstances;—that the finger of the surgeon, and the pipe of the injecting apparatus, however long, have very rarely, if ever, been found soiled with fecal matter when withdrawn;—that the membranous filaments which are sometimes found in the rectum, but rarely, if ever, in any other part of the intestinal canal, prove that the lining membrane must have been in contact, and the rectum empty for several hours;—that the sphincter muscles of the anus are considerably weakened in *prolapsus ani*;—that in the operation for *fistula in ano* they are completely divided, and cannot act as sphincters;—that not only these muscles, but also a portion of the rectum above them, are occasionally destroyed by "venereal, cancerous, and other ulcerative processes," yet it rarely happens that the power of retaining the feces is at all impaired;—and that, from numerous experiments, he found that on passing an œsophagus tube half an inch up the rectum, neither flatus nor feces escaped;—that about an inch and a half higher nothing escaped, but the tube could be moved about freely in a space ascertained to be the pouch of the rectum, perfectly open and empty;—and, *lastly*, that "from the highest part of the pouch to the upper extremity of the bowel it was found that the tube could not be passed upwards without meeting with considerable resistance," which continued to increase till the instrument reached the uppermost part of the rectum, when it passed rapidly forward as if through a ring, when "a rush of flatus, fluid feces, or both, took place through the tube." When this did not occur, there was a distinct feeling of the tube having entered a solid mass, and, on withdrawing the instrument, it was found

covered for a few inches at its upper extremity, and with its eyes plugged up with solid excrement—and that this appearance was always confined to the upper extremity.

PURE WATER.

One of the first subjects urged in the City Council by each of the three last Mayors of this city, if not by their predecessors, has been the supplying the city with pure water; and yet we are every year drinking water that is worse than ever. The citizens have generally, if not unanimously, responded to the sentiments of their Chief Magistrate, on this subject, and yet they are no nearer a supply, so far as we can perceive, than were their fathers a century ago. There is surely a want of energy somewhere in pursuing this subject; and if the present Mayor can succeed in *accomplishing* the object, he needs no other claim to the gratitude of his fellow citizens, or to a name that shall be imperishable. In *recommending* action on this subject, he has done just what his predecessors did—no more. We trust that the numberless official cares that will crowd upon him, will not prevent him from pursuing, until attained, an object most truly and literally of *vital* importance to his constituents.

The City Council of New York have solicited the Legislature of that State to empower them to negociate a loan of two and a half millions of dollars for a similar purpose, and in no other way can the people of that city get so much good out of that, or even double that sum.

Avoyelles' Parish, Marksville, Louisiana, Dec. 21, 1833.

The general health of New Orleans is good at this time, considering the number of its intemperate inhabitants. They have a small brush of a fashionable disease among them at this time, "Smallpox."

A Looker On.

We can never estimate the powers of *Ergot* too highly.—It at all times should be given in infusion, in frequent, repeated doses.

A Medical Man.

Coincidence of Human Epidemics with those among Fishes.—In 1784, several of the Indian tribes in the State of Massachusetts suffered very severely from a pestilential disease; and it was remarked that, during its prevalence, all the "blue-fish" had disappeared from the sea on that part of the coast. What is singular is, that since that period they have never returned in any numbers; and previously so abundant were they, that the fishing was a lucrative employment.

Every one has heard of the extraordinary mortality, in the year 1789, among the haddocks off the shores of Norway, Scotland, &c. The journals of the day give ample accounts of it. Captain Steward, who commanded a vessel from Archangel, tells us that the sea was covered, for several leagues in extent, with the floating dead fish. Now during this time, several parts of Scotland were ravaged with a most malignant scarlatina and cynanche. The reader will find some very interesting details in Sinclair's Statistical Reports. One of the most memorable years, for the coincidence of several of Nature's dismal disasters, was 1756. Lisbon was rent to pieces; the island of Mytilene, in the Archipelago, was shaken to its centre; North and South America felt the heavings of a general earthquake, and Ausio was literally annihilated. Ætna and Hecla were lighted up with double fury, and England, France, and other

countries were ravaged by a formidable epidemic of putrid sore throat, and Constantinople and the East by a destructive plague. The same year was remarkable for the vast numbers of whales and other fish found dead on various parts of the ocean. Again, in 1775, the State of Connecticut, in North America, was visited by a very fatal dysentery and by a cynanche maligna; and the waters of the harbor of Wellfleet, near Cape Cod, were so affected, that all the oysters died—vide Dr. Webster's interesting work. A similar mortality among oysters has been known to accompany severe epidemics of the yellow fever, as in the years 1793, 4, and 6. The old author Cedrenus relates that, during the dreadful pestilence which brooded over almost the whole earth, in the latter part of the sixth century, a vast quantity of fish died in many places; and we read in the Universal History of Magdeburg, and in the tenth book of Baronius, that "a pestilence which was truly most fatal to the human species, was no less so to aquatic animals; for the banks of rivers were covered with dead fish, which putrefied and infected the air with an intolerable stench." Nicephorus and Echard mention that there was a terrible mortality among whales, about the middle of the fifth century, when the Roman Empire, under the reign of Theodosius, was desolated by a plague. It was believed that these monsters of the deep had been battling with each other; but such glorious results of war appertain to man alone!

Alibert in Revue Medicale.

Tonospasmia, a new Nervous Disease.—Dr. Semmola of Naples has recently published an account of a very singular spasmodic disease, which he observed in the hospital of incurables there. It occurred in a young man, of an apparently healthy constitution. As long as he remained quiet, without speaking, there seemed to be nothing the matter with him; but no sooner had he uttered any sound, than he was forthwith seized with violent general convulsions;—this dependence of the spasms on the voice induced Dr. S. to designate the disease "*tonospasmia.*"

If the patient persisted to speak or to cry out, the spasms continued; and, if he ceased, they ceased also, leaving him perfectly well. The muscles chiefly affected were the extensors of the neck, trunk, and extremities; and the character of the spasmodic movements was, that the legs and arms, after frequent and irregular involuntary convulsions, were suddenly extended; the legs being at the same time brought in contact with and pressed against each other, and the arms forcibly applied to the side. The author compares them to the movements of a frog when submitted to the galvanic action. The malady was only of three days duration when Dr. S. saw the patient; and his health was so good, that he was able to follow his occupation of a porter, provided always that he remained quite taciturn.

Fear seemed to be an occasional exciting cause of the convulsions. The proximate cause Dr. S. supposed to be a "hypersthenia, or irritation of that part of the mesocephalon, from which the extreme roots of the recurrent nerves are given off, in company with the nerves of motion," producing an irresistible association, or sympathy betwen the muscular movements on which the voice depends, and the general convulsions of the body. His prognosis was therefore favorable; and the treatment which he adopted was depletory; viz. bleeding from the arm, and the application of leeches to the mastoid processes. The patient was very speedily quite cured.—*Annali Universali.*

Medical School of Kentucky.—There are 260 in the present Medical Class at Transylvania. Of these, 106 are from Kentucky, 39 from Tennessee, 25 from Alabama, 20 from Virginia, 19 from Mississippi, 11 from Georgia, 10 from South Carolina, 7 from North Carolina, 7 from Illinois, 6 from Ohio, 4 from Missouri, 3 from Indiana, 2 from Louisiana, 1 from Pennsylvania, 1 from Florida.

We acknowledge the receipt of Morten's Illustrations of Pulmonary Consumption, and shall take an early opportunity to notice the work as it deserves.

Whole number of deaths in Boston for the week ending February 1, 25. Males, 17—Females, 8. Of old age—consumption, 3—infantile, 3—croup, 1—scrofula, 2—inflammation on the lungs, 1—sudden, 1—intemperance, 1—typhous fever, 4—quinsy, 1—complaint of the kidneys, 1—rash, 1—scarlet fever, 1—fall, 1—apoplexy, 1. Stillborn, 3.

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